PATHOLOGICAL EVOLUTION.

PATHOLOGY FROM AN "EVOLUTION" POINT OF VIEW, BY J. H. MEIRING BECK, M.B., C.M., M.R.C.P., ED.

[Read 1885, May 27.]

I may be excused if I approach my subject with considerable hesitation. Its importance on the one hand, and its difficulties on the other, are sufficiently great to induce the greatest diffidence in bringing it forward. There are reasons why here in this country, however, where to a large extent we have very special opportunities for the investigation of disease phenomena, where, through comparative isolation of the inhabitants, it becomes less difficult to trace the development of disease, its progress, and means of arrest—there are reasons why here particularly the question should be regarded with the greatest interest.

Since modern medical thought has begun to busy itself with the "Germ basis" of disease, and since it has become almost universally accepted as a part of medical dogma that most of the diseases we have to deal with have their origin in living germs, in organisms having for their regulation the same laws that guide living matter in other directions—since the modern medical mind has commenced to realise this great fact, methods of reasoning which before would have been considered inadmissible have more and more begun to force themselves as proper, and in the highest degree applicable.

In order to render excusable my object, it is necessary that I should, in a concrete way, attempt to convey to the minds of those here what really I consider the relationships existing between the *Doctrine of Evolution* and the development of disease, and further that I should in the same concrete manner attempt to convey in how far human knowledge will be advanced by a demonstration of the fact of such application.

My theory as it stands might be postulated briefly, and simply thus:

Most diseases with which we are acquainted have their origin in tangible and living germs, demonstrable in many instances by the microscope to the sense of sight.

These germs are organisms which are low in the scale of life, and have an extremely short life history.

A germ produced from a pre-existing germ at this moment may, before many hours have passed, have given rise to others, which again in their turn, and in an equally short space of time, may give rise to further and more successive crops. This may be demonstrated experimentally—*e.g.*: If into a rabbit, a small quantity of septicœmic fluid be infected, in a few days' time the blood will be found teeming with millions of minute organisms, which go on multiplying till probably they so drain the sources of food supply which are necessary for the maintenance of life, in the various living cells which go to make up the various organs of the rabbit, that death of the whole animal ensues.

Being low organisms, and having a short life history, many generations may succeed each other rapidly, and a consequent possibility follows that surrounding circumstances may modify appreciably and rapidly the constitution and organization of these germs, so that it is possible for a germ, harmless now, to assume extremely virulent properties soon.

That this is no mere speculation but actual fact, Pasteur has demonstrated experimentally. His results are too well known to render it necessary for me to do more than briefly indicate what they prove. He experimented largely with the germs that are associated with "splenic fever" in sheep. This, an extremely infectious and fatal disease, attacking sheep in various countries, Pasteur found was caused by a germ which could be changed completely by simply cultivating it in certain fluids. Starting with a virulent germ, he found that after cultivating about 30 or 40 generations in such fluids, he obtained a changed organism, unmistakeably derived from the harmful parent, but quite incapable of exercising its functions, in other words, quite incapable of causing the same virulent type of disease that the parent germ could.

Here, actually in an artificial laboratory, it was possible to create changes in an organism sufficiently tangible to be appreciable by ordinary observation, and at least this *fact* then is proved : that the germs we deal with in disease are *not fixed* in character, and that if they could be experimentally modified, then in Nature's laboratories existing around us far more exquisite modifications may be possible.

The fields for speculation opened up by Pasteur's results are as wide as they are beautiful, and I do not say too much when I declare that they usher in a completely new epoch in the history and practice of medicine.

We have seen that we deal with living organisms of a low order, demonstrated by Pasteur to be changeable in *type*.

May we not go one step further, and speculate also as to their changeability of *species*. Up to this I am perfectly aware this question has neither been much discussed nor much entertained, and it is just one of those speculations which does not admit of proof at present.

Because it has not been demonstrated that *higher organisms* have ever changed their species, the conclusion at once is rushed at that such a thing as modification of species is absurd.

"De Quatrefages," in an admirable book on the "Human Species," published not long ago, devotes 500 or 600 pages to an attempt to prove the unscience (if I may so call it) of such an assumption, and though expressing the greatest admiration for Darwin, he criticises the assumption of "*Origin of Species*" in terms of the greatest severity. He bases his antagonism to Darwin's hypothesis almost entirely upon the assumption that no fertile hybrid has ever resulted from the crossing of plants or animals of different species. Now, it is not for me to say anything in this connection. I have, however, taken the trouble to look into the matter, and I have the good fortune to be able to quote from papers which appeared in the "*Botanische Jahrbücher*," and in the "Naturforscher," by Herr W. O. Focke, observations which at least do not render this fact quite so certain as "De Quatrefages" would make out.

Focke studied particularly the behaviour of different species of "blackberry" existing in Europe. Since 1857, *i.e.*, prior to the publication of Darwin's great work, he has devoted study to this group of plants, and this is what he says:

"That the blackberries do in fact very frequently produce hybrids is certain. Rubus Cœsius a well-known variety of the plant fertilises all other species with which it occurs in common, and like various other species is accompanied by its hybrid progeny.

"It has often been doubted whether permanent species can arise from hybrids.

"Hybrids between species mutually remote from each other are often sparingly fruitful. But we often find, *e.g.*, in Rubus Cœsius, and R. Tomentosus (two well-known species of blackberry), in favourable localities, all intermediate links between sterile and fairly fruitful specimens. The original lack of permanence in hybrids, as numerous observations prove, *loses itself often entirely in successive generations.*"

He further on adds: "If we consider that the majority of our cultivated plants have been produced by crossing, whilst all our art and all our exaggerated influences of soil and climate have not been able to affect much change in given natural species, we shall not be able to resist the conviction that the crossing of species and races has a greater effect in the formation of *new* species than has hitherto been credited."

Now, I have quoted the above simply to show that the whole question of "*Origin of Species*" is still on debatable ground. That being so, surely De Quatrefages has fallen into error, when, instead of beginning his study with the lowest of organisms, he goes to the other extreme, and studies the "highest" for proof of his position.

Now it would be manifestly rash to jump to a positive conclusion with regard to at present an insufficiently proved assumption, the origin of one species from another, but this I do submit, that in a study of disease phenomena, and in a close observation of the behaviour of the low organisms associated with them, lies a possibility of a solution of this difficult question which is not properly appreciated, and the proud possibility rests with students of the conditions of life of these low organisms, in other words, with students of "Modern Pathology," to supply the links wanting in the admirable chain woven by Darwin and other great workers on his lines.

In this lies the value of the work, and *in this* lies the positive addition which it may be possible for students of modern pathology to make to human knowledge.

Compare for a moment how favourably situated our "disease germs" are for study in this connection, as opposed to higher organisms. Take man—an organism made up of infinitely numerous parts, every organ composed of an infinite series of living cells—consider what must happen before a change even of type is possible, to say nothing of a change which will permanently perpetuate itself in the offspring. Why, infinite generations would be required before a type differentiated from the present could become permanent and perpetuate itself, and infinite generations would comprise for man a number of years not measurable by ordinary human calculation.

In our "disease germs" we have on the other hand a "simple organism," differentiated, perhaps, not even as highly as the individual "cell" in any one human organ. Not only is this organism low in the scale of life, but it has a power of multiplication which renders infinite generations possible in an extremely short time. A few days suffice to supply us in this connection with as much scope for observation as thousands of years in connection with higher organisms.

More than a year ago, in a communication I made to the South African Medical Society, I contended for a possible *de novo* origin of small-pox. More than *two years ago* I read before the members of this Society a paper in which I attempted to explain the cause of the camp fever of Kimberley.

In both papers I followed a line of reasoning which assumed the possibility of the development of "disease germs" where before none existed, and where special conditions had arisen to favour their development.

Since then I have closely watched such disease phenomena as have come under my notice, and my observations have only strengthened my conviction as to the variability of germs.

Last year an epidemic of pneumonia occurred in Worcester, where I at that time practised.

It is not necessary for me to go into modern ideas with regard to this disease, but I may by way of explanation say that a large school of pathologists now regard it no longer as a disease of the "lungs" proper, but as a fever having for its distinctive character certain changes in the lungs, just as in the same way small-pox is a fever which has for its distinctive charater certain changes in the *skin*. At the same time with this epidemic occurred an epidemic of *rheumatic fever* and an epidemic of *remittent fever*.

The last we all recognise as a malarious fever. Rheumatism, Maclagan, one the greatest authorities at the present time on the subject, and the collaborator of the *salicin* treatment of the disease, declares to be also malarious in origin. If we agree with him, and I for one do agree with him, then the remittent and rheumatic fevers must have more or less allied conditions causing them, and be subject more or less to the same casual laws. Now in one house a man was attacked with pneumonia, his wife with remittent fever,—both fell ill at the same time, both had typical attacks. The wife suffered from chest complications of a decided kind, a significant fact when taken in connection with the pneumonia of the husband. These passed off after a while. In another house one child developed pneumonia, another child rheumatic fever. Both fell ill at the same time, both had typical attacks. The thought suggested itself very forcibly that the simultaneous occurrence of a pneumonia in the one house with the malarious remittent fever, in another house with the malarious rheumatic fever, was an indication that, in some way or other, there was an associated causal condition for the three different diseases.

At Kimberley I am assured, on the best of authority, that of Miss Schreiner, a lady of the most remarkable powers of observation, who devotes her entire energies to the care of the sick, and charitable work of a like kind, that the natives almost invariably develope "pneumonia" after a heavy fall of rain.

Now it is well-known that natives, almost everywhere, are not extremely susceptible to ordinary malaria, and the thought occurs that here, under circumstances unfavourable in the native to the development of an ordinary malarious fever, conditions which are known to be favourable for the development of the malarious germ become favourable for the production of pneumonia. I mention this by way of additional evidence in support of my hint as to the probable identity of casual relationships for some forms of pneumonia and malarious fevers.

Now I do not positively say that this casual relationship exists, but if so, and if all three are, as we have every reason to believe they are, germ diseases, then we may fairly assume that my Worcester cases were a practical indication of the possibility of germ transmutation.

In other words, the conclusion is suggested that germs bred under certain related conditions, finding dissimilar circumstances in different subjects, might be assumed to possess the potential power to develope in different directions, and cause in different individuals different results.

This is a wild speculation, it may be said, and so perhaps it is. Facts, however, are facts, and from the facts I have above adduced, my deduction is, I maintain, rendered probable. At least, it will be conceded that there may be truth in it, and if true, a very different realisation of the relationships existing between disease phenomena, hitherto regarded as having no relationships, will be opened up.

I maintain that with the advantages of this country for clinical study, questions of this sort are of a kind which ought to come up, and if some thought be induced in the direction I have indicated, I shall not have come forward with my theories, crudely developed as they are, in vain. The whole subject is full of practical application, and it would require more time than I can give, or than it would be right to expect you to give, if I were to enter fully into this aspect of the matter. I may, however, briefly be allowed to show by a single illustration the kind of application possible. To pursue this matter exhaustively is not necessary.

All medical men in practice will have come across many cases which, as they go on, change their type.

For instance, in the course of an ordinary pneumonia, a typhoid condition supervenes, or in the course of an ordinary fever a sudden pneumonia developes, and perhaps carries off the patient. How perfectly explainable this becomes when we assume the possibility of a change in the organisation of the germ which constituted the original infecting factor. For various reasons, the *pabulum* in the body at the time of infection preferred by one germ may become modified, or may become exhausted.

One of two results must follow, either the germ must die or develope an aptitude for changed circumstances of life. This in so low an organism as Pasteur has shown does not occur without some change in its character, and change in character creates modified result. As I have said, the subject is full of practical application, and I venture to predict that in this direction lies the greatest possiblity and probability for future pathological advance.

It would be out of place for me to burden my communication with an enumeration of clinical observations. I must not forget that I am addressing a mixed audience, whose indulgence I may tempt too much. To some extent I could not avoid doing this, however, and I may be allowed to express a hope that the slight technicalities adduced may not have been without sufficient general interest to justify them.

It will be noticed that I have in my remarks touched upon the "evolution hypothesis" only in as far as it bears upon the external agencies in disease.

I have left untouched its application as regards the internal mechanism. The mechanism by which "predispositions," hereditary or otherwise, are determined, and the agencies by which "resisting power" to infection is developed, would fall under this head. The living body, it must be remembered, is made up of living cell elements.

These obey the same laws that living matter elsewhere does, and a constant adaption to surrounding circumstances goes on. That this is so is beautifully illustrated by the "official documents" upon the annual mortality in the thousand inhabitants at Sierra Leone from 1829 to 1836.

From these it appears that while 410·2 per 1,000 Europeans annually die from "marsh fever," only 2·4 Negroes succumb. This will amply demonstrate what I mean. On no assumption can the disproportion in mortality be explained but that by which the Negro is credited with a special resistance to miasmatic infection.

This power of resistance can only have resulted from a gradual adaptation to their surroundings of the living cells in successive generations of Negroes, an adaptation which in transmission from father to son became intensified sufficiently to create ultimately an almost absolute immunity from marsh fever.

On the assumption of such adaptation can almost be explained the occurrence of acute fevers in epidemic form, such as, *e.g.*, small-pox. It is reasonable to assume that when a series of laws, climatic or otherwise, come into operation to favour the development in certain directions of germs outside, that the same laws, reacting upon the human organism, create an adaptation of parts, which for the time determines a susceptibility to infection.

When these laws change, a reverse process may be assumed to go on, and the epidemics disappear.

If this were not so, then it would be quite unexplainable why such epidemics as our last small-pox outbreak should ever disappear entirely. We know that every person attacked increases in a positive ratio the quantity of poison. When, therefore, an epidemic is at its height, the quantity of infecting material must also be at its maximum.

Instead of going on, however, a retrogressive development occurs, and the epidemic dies out not only, but also remains away for perhaps a period of years, and then appears again. If this retrogressive development were not there, we should be at an entire loss to account for the fact that there was no spread of small-pox in Cape Town last year when cases from Kimberley were imported, especially when we take this fact in connection with what happened in 1882, when a single case created in the most rapid manner an epidemic which raged over the whole of the Western Province almost. It must be remembered that there are always unvaccinated persons in large towns.

M. Boudin, in an interesting work on ethnological pathology, writes as follows:—"Elephantiasis, that affection by which certain parts of the body are sometimes deformed in so strange a manner, is found in the Indies and at Barbadoes.

"In the latter island negroes alone were attacked by this hideous disease till the year 1704. *One white* was in that year affected by it for the first time. But the disease made way, and in 1760 it had extended to the Creole population.

"Whites of *European origin* have as yet escaped.

"The elephantiasis of India is found in Ceylon. There, again, it only attacks natives, Creoles, and individuals of mixed blood. Hindoos and Europeans are exempt from it. *Only one case* of this disease had been observed in a European white.

"But this individual had inhabited the island for thirty years." Acclimatisation, he adds, had been carried so far in his case as to cause him to lose his ethnological immunity. In other words, this acclimatisation in some way or other must have modified the internal mechanism of this European sufficiently to create a susceptibility to disease for him which before did not exist. Only on this assumption can this fact be explained.

It would be interesting to pursue this part of my subject further. I have already, however, exceeded the time I have a right to expect you to give me. I shall, therefore, content myself with the hope that I have said enough to demonstrate how important it is for us to recognise in pathology, as in other sciences and departments of thought, the great fundamental principle indicated by the "Law of Evolution," and how, on the assumption of its applicability to questions of disease, what we now regard as abnormal processes may be brought into normality and unity with everything around them.

AN ADDENDUM TO A PREVIOUS PAPER ENTITLED "PATHOLOGY," FROM AN "EVOLUTION POINT OF VIEW."

(Read at the May Meeting of the South African Philosophical Society, 1885.)

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It was not my intention this evening again to bring forward the subject which at the last meeting of this Society I had the honour to introduce. One or two of the members present then, who were interested in the remarks I made, however came to me after the meeting, and requested me to write an addendum to my paper, inasmuch as it was thought that by so doing I should sustain, or at any rate revive whatever little interest was roused then. I hope therefore that you will bear with me, and that if I demand from you what may be considered perhaps too much indulgence, you will kindly lay the blame not so much at my door as at that of the gentlemen responsible for my communication of this evening. I shall try to be as brief as possible.

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It will be remembered that I divided my subject into two parts. In the first I dealt with the "Exciting factors"—the germs of disease, and endeavoured to explain how I thought the law of evolution influenced them, as I believe it influences other living organisms, and how as clinical students we were fortunate in having phenomena to observe, associated with living forms so low in the scale of life, and possessing so remarkable and rapid a power of multiplication.

In the second portion I endeavoured to shew briefly how the same laws acting upon the living human organism, and its component cells, might also be inferred to influence *them* so as to establish as it were a receptivity for, or resistance to, this exciting factor.